

## CORRESPONDENCE

To the Editor,

**Chance Coincidence in the Cross-Correspondences**

Any attempt to interpret the Cross-Correspondences (C-Cs) anew is to be welcomed, if only to re-arouse interest in this seriously neglected and complex field of ostensible discarnate communication. However, Christopher Moreman's effort to show that chance coincidence can produce correspondences as significant as those typical of the C-Cs is fundamentally unsound.

Let us first acknowledge that Moreman's experiment has shown that correspondences of some sort can indeed be found in a randomised selection of literary extracts. The questions which arise are whether these extracts are meaningful, in the manner of many of the Cross-Correspondences, whether the experiment is truly relevant to the Cross-Correspondences, and whether he has drawn the right conclusions from the most persuasive cases. All answers are in the negative.

Moreman early acknowledges that specific instructions, like that of the supposed discarnate Edmund Gurney to Mrs Holland to send her script to someone she did not know to an address she had not heard of (5 Selwyn Gardens, Cambridge), must pre-suppose telepathy of some sort (although even this is stretching the human telepathic postulate somewhat). In extensive correspondence the first author had with Moreman following the presentation of his experiment to the SPR's international conference in 2001, he accepted that information psychically received by mediums could account for some of the communications, but contended that the discovery of meaningful correspondences was nevertheless entirely attributable to the zeal and imagination of the investigators.

This raises some difficult issues. Whence did Mrs Holland derive her written instruction? Was it from the same source that casually mentioned the circumstances of the life, death, age, burial place, etc. of a John Collins, facts which were not to be largely validated until some thorough burrowing work by Guy Lambert (1967) about half a century later? The only non-discarnate source must be the mind of Mrs Verrall, to whom the message had to be sent. But are we to believe that Mrs Verrall in Cambridge sent a telepathic message to someone in India whom she didn't know about a matter she could not have been aware of, in order to initiate a cross-correspondence of whose outcome she was ignorant? These are not idle questions. The admission of telepathy as a contributory element undermines the whole foundation on which the random coincidence hypothesis is based.

In fact Moreman leaves us with an arbitrary mixture of random, coincidental similarity and psi-produced intelligence, with no means to determine which is which. But from the design of the experiment, the sole conclusion it is permissible to derive from it is that random coincidence alone can account for whatever similarities can imaginatively be coaxed from the texts. And indeed that is how Moreman concludes his paper, quietly burying the psi content he has earlier invoked in order to account for a range of information in the name-and-address category.

That really will not do. As the first author wrote to Moreman in 2001, "You really cannot have it both ways: you cannot draw a conclusion based entirely on chance, while finding yourself obliged to postulate a no-chance explanation."

There are many comparable examples of requests via the principal mediums' scripts or oral messages either to check this, refer back to that or to inform the other. Mrs Verrall, for example, was specifically urged to send her early script to Dr Hodgson because he was one of the few people considered likely to recognise the significance of a reference to *Syringa* (lilac) (Verrall, 1906). Indeed, what chiefly characterises the best C-Cs is that they form a pattern and sometimes convey a message. But there is nothing remotely akin to a message in Moreman's test. Many of the real C-Cs are presaged or interlarded with communications responsive to the sitters' requests. This is a feature wholly and necessarily absent from the random scripts. In any event the Moreman experiment wrenches the C-Cs out of their context. Many scripts contain extensive interchanges between the communicator and the medium, and often between communicator, medium and sitter or interrogator: exchanges that are coherent, meaningful and relevant. The personalities of the communicators are generally consistent, idiosyncratic and recognisable. The Moreman scripts are devoid of all such features.

Equally pertinent are other aspects of the C-Cs which make the random phrase or word experiment otiose. We have many repetitions or reiterations in the same scripts, not least when the medium appears to have difficulty in either hearing, understanding or spelling the word or phrase transmitted.

I omit the vast and complex evidence to show where and how scripts are predictive of events to come, but this is a field in which Professor Archie Roy and I have been labouring for some years, and whose outcome we expect to present to members in the not-too-distant future.

A glance at Moreman's list of references reveals an odd and disturbing fact. Just as it would be inexcusable to attempt a history of World War II without mentioning Churchill, or write a biography of Wellington while avoiding any reference to Napoleon, so it is of little value to assess the evidential value and the coincidental significance of the C-Cs while failing to cite, let alone explain, the Countess of Balfour's examination of the Palm Sunday case (Balfour, 1960), the most authoritative and least vulnerable account of the accumulation by more than one medium of a coherent evidential story, well summarised by Rosalind Heywood (1960) for those reluctant to immerse themselves in the elaborate detail of the main study. As far as I know that paper failed to produce a single criticism which might undermine its authenticity. None of the earlier authorities on the C-Cs was able to take this late work into account, apart from Salter, whose valuable study, *Zoar* (1961), Moreman cites, but it makes only a cursory reference to the case, which Salter had dealt with in detail elsewhere (Salter, 1948, 1960).

It is not without significance that, in citing the work and implicitly criticising the gullibility of leading researchers, Moreman makes no mention of the seminal study by Gerald Balfour on the psychological aspects of Mrs Willett's mediumship (Balfour, 1935). He also unaccountably ignores Salter's account of the Rose of Sharon case (Salter, 1963), which is good enough evidence to show

how misconceived is the notion that odd concordances from disparate sources can begin to explain the abundance of pointers to a set of future events between a few days and five years before they occurred.

Nor—still more disturbingly for Moreman's thesis—can his experiment be reconciled with two other types of cross-correspondences not derived from the familiar group of contributing mediums, nor analysed by the supposedly credulous SPR leaders. The first is the much overlooked but potent example of a cross-correspondence apparently designed by the deceased brother Walter of the remarkable and still controversial American medium, 'Margery' (Mina Crandon). Here messages were given simultaneously, ostensibly by Walter, to three different mediums in Boston, New York and Niagara Falls, in the form of drawings, geometrical figures and in some cases Chinese characters. Verification was obtained by telephone and telegraph, enabling the entire message to be deciphered as a whole. In another case, Walter announced that Margery would make up a message—which she did after passing into a light trance—and that the remainder of (and solution to) the complete message would shortly be forthcoming from simultaneous reports through the other two mediums (Bird, 1929). Here was a straightforward and relatively uncomplicated example, arranged—indeed stage-managed—by the discarnate communicator working in close liaison with three sets of experimenters supervising three mediums, all of them conveying a cryptic segment of an ultimately coherent communication. Then there are the five examples cited by the eminent French savant Dr Gustave Geley (1914), on which Piddington (1916) comments. Geley's complaint, like that of Moreman and many others, was that the British C-Cs were needlessly obscure. Geley's argument was not that apparent C-Cs were no more than the product of inflamed imagination and Rorschach-animated philology, but that they did not have the admirable simplicity of the Gallic examples. Geley was certainly not suggesting that they could be dismissed as chance.

But, as Piddington gently observed, "If ten automatists on the same day each wrote 'A covey of elephants danced round Buckingham Palace' and sang 'We Won't go Home till Morning', no one could prove that chance would not cover the coincidences." One has to judge in these matters by common sense, and common sense does not lead everyone to the same conclusions.

We fear it may have led Mr Moreman astray.

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Christopher Moreman replies:

I should like to begin by saying that I am pleased to respond to the letter from Keen and Roy in the hope that it might further the debate and spark more interest in the rarely scrutinised Cross-Correspondences. Further, while I do not find any severe criticism of my work in Keen and Roy's letter, there appear to be some misunderstandings that require correcting. In fact, Keen and Roy accept the main finding of my study—that random scripts can produce correspondences of a similar kind to those in the original C-Cs—though they deny that such a finding should be seen as quite so damaging.

The first issue raised by Keen and Roy is that my acceptance of the potential role of telepathy in ostensible mediumistic communications “undermines the whole foundation” of my study. Firstly, it is important to note that I do not accept only that telepathy might play a role, but that psi in any number of manifestations might do so, be it in the form of precognition, clairvoyance or whatever. In this way, there is no need to “stretch” our understanding (if, indeed, it can be said that we have one) of telepathy in the way suggested by Keen and Roy. These two critics move quickly into a deeper discussion of the debate over whether survival or Super-ESP might best account for the acquisition of paranormal information via a medium, which is a debate that extends far beyond the bounds of the specific question my paper addresses, and is one that is not strictly speaking germane to my study itself. The C-Cs, as evidence for survival, rest on the strength of the actual correspondences over and above any paranormally acquired information.

The C-Cs themselves were originally produced in order to confound what has become known as the Super-ESP hypothesis. Simply providing factual information via paranormal means is not enough for the conclusion that spirits are the source of the information when other forms of ESP are more parsimonious. I emphasise, then, that paranormally acquired information is not enough evidence for survival. This is precisely the reason that the C-Cs have been held in such esteem by proponents of the survivalist camp. The complex patterns discovered to be embedded within the multitude of scripts relating to C-Cs are, in fact, taken to be the strongest evidence against the Super-ESP hypothesis.

That said, my study was designed to find whether the patterns and meanings detected in the original scripts might or might not be the result of chance combined with the ingenuity of the investigators. Certainly, the design that I used permits more than one conclusion, though the results of my study have demonstrated only one. If my scripts had not produced similarly striking patterns to the original C-Cs, then the conclusion would have been quite different.

Keen and Roy state that "what chiefly characterises the best C-Cs is that they form a pattern and sometimes convey a message. But there is nothing remotely akin to a message in Moreman's test." It is clear that they both agree that my study does include patterns and correspondences akin to those of the C-Cs, but their problem lies with so-called messages that are sometimes conveyed. Turning to the problem of these messages, however, one becomes unsure as to what exactly Keen and Roy are referring to. The examples given immediately after the above statement again speak to questions about mediumship in general and not specifically to the correspondences themselves. Idiosyncratic styles of speech or writing are cited as examples of differences between mediumistic communication and my pseudo-scripts that allegedly make the latter otiose. While these certainly are recognised as unavoidable differences in style between the scripts, it is unclear how such differences might actually help to establish any kind of meaningful 'message.'

On a more pedantic level, Keen and Roy are critical of a perceived lacuna in my citations. Simply, a short paper such as the *JSPR* permits is not the place for a history of the C-Cs, and the examples I chose from the vast numbers of C-Cs were selected for their simplicity and ease of understanding. Any contrast between my study and a history of World War II is ridiculously unfair. Certainly, readers of the *Journal* are well advised to consult the many volumes of *JSPR* and *ProcSPR* that contain the original investigations. In the light of the criticism, however, I might make some brief comments.

The 'Palm Sunday' case is one of the most convoluted of C-Cs, combining all manner of allusion, symbol, and coincidence culled from innumerable scripts written over several decades. Jean Balfour, who wrote the main report, points out "that my material had to be picked out with much labour from the vast maze of scripts in which it was embedded" (Balfour, 1960, p.86). Readers will no doubt easily detect the kind of ingenuity in finding patterns among a mass of information of which I am suspicious.

Keen and Roy also cite the 'Rose of Sharon' case as "good enough evidence to show how misconceived is the notion that odd concordances from disparate sources can begin to explain [the C-Cs]". Salter, who published the case, is more humble in his appraisal of it, stating, "To many members of the society the connections that have been suggested between the scripts quoted and the facts . . . will appear far-fetched and unpalatable. [sic]" (Salter, 1963, p.21). Certainly, Salter himself thought them worthy as evidence, as do Keen and Roy. I am, however, one of those to whom they do appear far-fetched and implausible.

In the end, the more convoluted the correspondences have become, the less readily one can see the alleged patterns without the careful guidance of the investigator involved. What my paper has shown is that the basic patterns and meanings that appear in the original C-Cs can also be found easily in randomly selected passages. As I stated in the paper, I believe that, given longer and more numerous pseudo-scripts, ingenious investigators would find increasingly complex correspondences, just as has been done when analysing vast numbers of genuine scripts received over a long period of time. Certainly, the presentation of new evidence from Keen and Roy will be greatly welcomed, assuming they can avoid the pitfalls of ingenuity and *post hoc* analysis into which many other proponents of the C-Cs have fallen.

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To the Editor,

#### A Rasch Scaling Approach to Questionnaire Research in Parapsychology and Anomalistic Psychology

We were delighted with the recent article by Chris A. Roe (2003), in which the concept of *transliminality* (the hypothesized tendency for psychological material to cross thresholds into or out of consciousness) was utilized in a test of psi. As a complement to that article, we should like to point out that two measures used in the study should have been scored differently, and that this might have affected some of the results. In particular, Thalbourne's (1998) 29-item Transliminality Scale should always be scored with the Rasch (1960/1980) scaling scheme published by Lange, Thalbourne et al. (2000; cf. Houran et al., 2003). Roe correctly acknowledged the existence of this revised scale (termed the Revised Transliminlaity Scale) but it is not entirely correct to describe it as an "abbreviated form" of Thalbourne's original measure (Roe, 2003, p.286). It is true that the Rasch scaling scoring scheme scores only 17 of the 29 items on the scale because of responses biases related to age or gender, but the entire 29-item scale is administered in order to maintain the context of the 17 items that are actually counted. Readers are referred to Houran et al. (2003) for a detailed discussion and a copy of the full scale. In addition, Lange, Thalbourne et al. (2000) illustrated that the Rasch scoring scheme may yield *different* results in statistical analyses than those obtained with the original scoring scheme outlined by Thalbourne (1998). We hope this clarification aids researchers and inspires them to use the Revised Transliminality Scale in future parapsychological research as well as studies in other areas of anomalistic psychology. In this way, we may better understand why some studies yield significant effects of transliminality while others do not (similar to the classic sheep-goat effect: see Thalbourne & Houran, in press).

On a related note, Roe (2003, p.286) noted that he used an adaptation of Thalbourne's Sheep-Goat Scale (Thalbourne & Delin, 1993). We are also happy to report here that there is also a Rasch scaled form of this measure as well. Readers interested in using this superior scoring scheme for this test should consult Lange and Thalbourne (2002). However, the Rasch Australian Sheep-Goat Scale is arguably not as comprehensive as the Rasch version of Tobacyk's (1988) Revised Paranormal Belief Scale (RPBS, see: Lange, Irwin et al., 2000), which point to two types of paranormal belief seemingly related to slightly different control functions — Traditional Paranormal Beliefs and New Age Philosophy (see, for example, Houran & Lange, 2001; Houran et al., 2001).